

the subject. When my book was originally planned it was intended that it should be a monograph of the specimens of okapi contained in the national collection, and it thus became entered on our list as "the monograph on okapi."

More, no doubt, might be written about the specimens which I had under examination, and I should have, in some circumstances, been able to add to what the book contains; but the problems which arose in the course of my work could not, in many cases, be satisfactorily solved by the examination of the existing material.

We shall have to wait for new observations made upon fresh or living specimens for a solution of the question as to what are the characteristics of the male and female okapi respectively, what are their geographical variations, and whether there are distinct races or subspecies.

E. RAY LANKESTER.

29 Thurloe Place, South Kensington.

SIR E. RAY LANKESTER is correct in supposing that I was misled by the last paragraph of the preface to his work on the okapi into the belief that there had been or might be an additional volume of text to supplement the illustrations given in the volume under review. From private correspondence which passed between Sir E. Ray Lankester and myself about three years ago I was under the impression that the "text" alluded to was in existence, and perhaps I arrived too hastily at the conclusion that for reasons of economy it had been put aside because of the intervening publication of M. Jules Fraipont's work. The title "Monograph of the Okapi" to which Sir E. Ray Lankester refers as likely to mislead an appraiser of his work was not of my bestowal, but is the official title of this valuable and admirably produced volume. The illustrations are fully described; but I suppose what I missed, and what I hoped might still be forthcoming, were the deductions to be drawn from these illustrations as to the affinities and systematic position of Okapia: in short, a statement of Sir E. Ray Lankester's personal opinions. He is probably quite right to withhold these until something is known of the beast's musculature and intestines.

H. H. JOHNSTON.

The Dynamics of a Golf Ball.

WITH a view to reproduction in the forthcoming Life of the late Prof. Tait, I have just been editing his popular article on long driving, which appeared in the *Badminton Magazine* of March, 1896. On reading Sir J. J. Thomson's lecture, as published in *NATURE* of December 22, 1910, I was greatly struck with the strong resemblance between golf-ball paths worked out mathematically by Tait and the stream lines of the electrified particles in the ingenious experiment devised by Sir J. J. Thomson. A few of Tait's calculated curves were given in *NATURE*, vol. xlviii. (June 29, 1893); but better examples will be found in the second paper on the path of a rotating spherical projectile (*Trans. R.S.E.*, vol. xxxix., or *Scientific Papers*, vol. ii., p. 386) and in the article on long driving already mentioned.

By laborious arithmetical calculations, Tait and his assistant computer worked out a series of possible trajectories with various values for the transverse force due to the underspin, obtaining, among others, the kinked path which Tait had already demonstrated by undercutting a light rubber balloon. It is extremely interesting to see how the several types of curve figured by Tait for the same initial speed of projection, but varying degrees of underspin, are almost accurately reproduced by Sir J. J. Thomson's beautiful method of subjecting a stream of negatively charged particles to a suitable combination of electric and magnetic forces.

C. G. KNOTT.

Edinburgh University, January 2.

On the Simultaneity of Abruptly-beginning Magnetic Storms.

I was naturally much interested in Dr. Krogness's communication to *NATURE* of December 8, 1910 (p. 170), and wish to take this occasion to express my gratefulness to

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him for making known his criticisms on some of the results of my investigations on magnetic storms, as well as on those of Mr. Faris, where there is opportunity for reply. I am also glad that he has made his statements sufficiently direct, so as to admit of an equally direct answer.

Dr. Krogness first wishes to show that my conclusion, that even the sudden magnetic disturbances do not begin strictly at the same instant, but at measurably different times at various points on the earth, rests on insecure foundation; he would make it appear that it was based on but two cases, viz. the disturbance of May 8, 1902, and that of January 26, 1903. He will find a table (No. VIII.) in No. 2 of my researches (December, 1910, issue of *Terrestrial Magnetism and Atmospheric Electricity*) which summarises the data from thirty-eight abruptly-beginning disturbances between the years 1882 and 1909, thirty-four of which were available to me when the article was prepared which Dr. Krogness reviews (*loc. cit.*, pp. 19-20).

The table gives the date and approximate Greenwich mean civil time for each of these thirty-eight disturbances, next the number of observatories for which time data were available and the approximate portion of a complete circuit of the earth embraced by the contributing observatories. Then the value of x , or the time in minutes required by a disturbance to pass over one-fourth of a great circle, and in the following columns is given the approximate weight to be attached to any particular value of x , as determined from all circumstances involved, and the source from which the data have been obtained. A plus sign attached to x means that the disturbance progressed apparently in an eastwardly direction, as indicated by an increase in the Greenwich mean time of beginning at easterly stations over that at westerly ones. A minus value of x means, of course, the reverse. Nos. 35-38 were since added on the basis of data communicated by Mr. Faris (*loc. cit.*, pp. 213, 214).

Out of thirty-eight values of x , only ten, or about one-fourth, have the negative sign, so that three-fourths of the disturbances of the type here considered show an eastward progression at the times of beginning. In view of the greatly varying circumstances on which the figures are based—different observatories, different instruments, times scaled by different persons, different years, covering a period of two and a half times that of a sun-spot cycle—it is going to be difficult to explain the persistency of the plus sign by any such possible errors as Dr. Krogness points out, which, as a matter of fact, even he will hardly contend would be always in the same direction for every observatory, nor even necessarily always the same at the same station.

From this table the following results are derived:—

Weighted mean value of 28 plus values of x	= +1'65 minutes
" " 10 negative "	= -1'80 "
Weighted mean without regard to sign	= $\pm 1'69$ "
(Hence velocity of progression for average sudden disturbance, whether to the east or to the west, is 99 km./sec.)	
Weighted mean with regard to sign	= +0'74 minute
(Hence average algebraic velocity of eastwardly progression is 225 km./sec.)	

We thus get a velocity for the progression of a sudden disturbance on the order of 100 to 200 kilometres per second; hence, if a sudden disturbance passed around the earth completely it would take approximately between seven and three minutes. We are here, then, dealing apparently with a velocity of a greatly subordinate order (1/3000 to 1/1500) to that of electromagnetic waves, which would require but a tenth of a second to pass round the earth, and of cathode rays which would take on the order of a half-second.

Another line of argument set forth in my papers is based on the harmonic analysis of the typical disturbance here under consideration, for which the effect, in general, is an increase in H (horizontal intensity) over the whole earth and a decrease in Z (vertical intensity) in the northern magnetic hemisphere and an increase in Z in the southern. It was found, for example, that the disturbance system of

May 8, 1902, was a two-fold one: first, the stronger, a set of electric currents which, if negative, circulate in the upper regions around the earth eastwardly (anti-clockwise) if one were looking down on the North Pole, and secondly, a weaker system, imbedded within the earth, possessing the characteristics of directly induced magnetism. It is a matter of interest that the harmonic analysis prescribes the same direction of progression around the earth for the upper negative electric currents as has been revealed by the generally eastwardly progression of the times of beginning; and it is natural, then, to inquire whether these overhead negative currents consist of negative ions moving at the rate of 100 to 200 kilometres per second, the resulting effect of which on our magnetic needles is merely an exhibition of the Rowland effect on a scale far transcending any laboratory experiment.

We have found that the speed of these negative charges must be on the order of about 1/500 that of cathode rays. My provisional calculation showed that if we are dealing here with moving ionic charges, then at the height of about 75 kilometres the rarefaction of the air and the other necessary conditions, so far as can be judged from surface experiments, would be such as give a velocity of the order required to satisfy the apparently slow propagation of magnetic effects over the earth. The lower the current gets down the slower the speed, and, if other things are equal, the greater the effect. Whether this is in accordance with actual observation is at present undergoing an examination.

Now let us look briefly at the matter in another way. Suppose a negative ion is set in motion at a given altitude and in an easterly direction; the deflecting effect of the earth's magnetic field on the eastwardly moving negative charge is to bring it down closer to the earth. But, as we have seen, the ionic velocity decreases with decrease of altitude, and hence the magnetic effect produced by the moving charge on a needle at the surface would begin later and later as the charge travelled eastward. If, on the other hand, the negative charge started westward around our planet, then the deflecting effect of the earth's magnetic field would be to make the charge move higher and higher or faster and faster. We might thus possibly have the following state of things: due to some cause, electric charges are set in motion in every direction from a given point overhead. Those with an easterly component of motion would have their velocities checked in the manner just described, whereas those having a westerly component would have them increased, so that for two stations, one east and one west, the magnetic effect might be recorded later at the east station than at the west one—as we have actually found to have been the case in the vast majority of the thirty-eight cases above treated.

Dr. Krogness next attempts to break down the testimony regarding non-simultaneity of commencements of sudden storms furnished by Mr. Faris (*loc. cit.*, pp. 93-105). Dr. Krogness notwithstanding, Mr. Faris *does* make a statement (p. 98) as to his method of time scalings and the various matters involved to secure the desired accuracy. It is the custom in the Coast and Geodetic Survey to take into account every possible source of error, and as the result Mr. Faris says:—"It would thus appear that with especial care the times could be scaled from the magnetic records within one-half minute in any individual case."¹ He furthermore states (p. 105):—"In closing, it seems proper to state that the scaling of the times of the beginnings of sudden impulses is not so difficult a matter as it is to ascertain the exact correspondences in the curves at different stations, for the form of the photographic record of the starting impulse is not always exactly the same at different places; that is to say, the fixing of the exact point of the beginning of the disturbance is sometimes more difficult than the reading of the time after the point is decided upon. This difficulty arises chiefly from the fact that the magnetic traces, except in tropical latitudes, are much of the time not smooth curves."

¹ In the December, 1910, issue (*loc. cit.*) Mr. Faris has two communications which will give further evidence on the matter of accuracy of his time scalings to which Dr. Krogness may be referred.

This matter of being sure of having precisely the same perturbation for all stations is one apparently insufficiently considered by Dr. Krogness. For example, he questions our time of beginning in the H disturbance for the storm of May 8, 1902, as recorded at Potsdam. I gave 12h. om., and he gets 11h. 58m.; I have had our scalings gone over once more, and have this to say: unless the Potsdam Observatory has revised the data furnished us (copy of magnetogram and accompanying time data), the time given by Dr. Krogness is wrong, and 12h. om. is correct. If our Potsdam data are correct, then Dr. Krogness has either made an error somewhere, e.g. may not have considered the fact that the middle of the hour breaks in the Potsdam curves is for local mean time, not for middle European time, or he has taken a small preliminary tremor observed at some of the stations, but of a different character than the particular perturbation considered. He should also remember we had before us the curves of twenty-five observatories, with the aid of which the identical characteristic point could be determined upon for each, so far as that is possible.

Another fundamental fact in terrestrial magnetism of which Dr. Krogness is not aware is this: the existence or non-existence of a terrestrial magnetic phenomenon cannot be proved by *one* magnetic observatory, no matter how excellent and superior its equipment may be—not even the whole European group, consisting of about twenty magnetic observatories, would in certain instances suffice. Since the publication of the papers criticised by Dr. Krogness, a prediction which I made has been found true. On p. 25, *loc. cit.*, I say:—

"In fact, I confidently expect, as soon as a complete analysis has been made of magnetic disturbances covering the greater portion of the earth, it will be found that the disturbance field, in general, presents all the same characteristics of the terrestrial, primary one—the disturbances will themselves reveal effects from terrestrial, continental, regional, and even local causes (earth currents, for example, whose path and intensity depend upon local character of soil, &c.)."

Mr. Faris has brought together for the March, 1911, issue (*loc. cit.*), the data from observatories all over the globe with respect to some peculiar magnetic disturbances which occurred between December 29 and 31, 1908. With his permission I will anticipate by saying that these disturbances, of which there were eight cases, occurred each time over restricted regions of the globe—e.g. in the United States and not in Europe, or *vice versa*, &c. The interval between the occurrence of the disturbance in the United States and Asia, or Asia and Europe, was not a matter of a few minutes, but a matter of many hours! Though this disturbance—whenever it occurred—never lasted much more than half an hour, and was during an otherwise magnetically calm day, nevertheless a number of observatory directors are on record as having recognised it and having characterised the day as disturbed (class 1). The interesting point is, however, that they did not all get it at the same absolute time, but at times differing by many hours! A discussion will be given in the March issue (*loc. cit.*).

Hence, by attempting to disprove a fact based on such extensive data as referred to above, with the aid of data at *one* observatory—Potsdam—Dr. Krogness has simply shown that he is unfamiliar with a fundamental fact regarding the *distribution* of magnetic phenomena. Every magnetic phenomenon known to me partakes of a most complex character, and to get a general result of value it is necessary to base an investigation, not simply upon one station or one part of the earth, but on as great a portion of the earth as possible—the greater the better.

Dr. Krogness next reverts to the disturbance of January 26, 1903, the times for which were scaled by Prof. Birke-land. He exhibits a rather interesting method of discrimination between the various stations, and appears to have overlooked where his own figures lead. He rejects *in toto* the three Coast and Geodetic Survey magnetic observatories, Honolulu, Baldwin, and Cheltenham—the latter two probably because Prof. Birke-land had found the identification of the point of beginning of the disturbance

difficult. But this Prof. Birkeland says was likewise true of Toronto, yet Dr. Krogness retains this station; why he rejects Honolulu Dr. Krogness does not say. Again, he overlooks the fact that when he corrects Birkeland's scaling for San Fernando he has improved the easterly progression—Prof. Birkeland's value was nearly two minutes too high. In view of the uncertainties in Prof. Birkeland's scalings revealed by Dr. Krogness, and as Prof. Birkeland fails to specify the particular element considered, not full weight could be attached to this disturbance in the above table. It should also be stated here that Prof. Birkeland considered, in all, six characteristic points of the disturbance curve, and my result was based on *all* the scalings—seventy-two in number—and not merely the half-dozen taken by Dr. Krogness. Did I myself consider such limited data as Dr. Krogness uses adequate for the purpose, I might point out that his own figures show an easterly progression of the times on the order of what is to be expected, which would have been still further accentuated had he not rejected Honolulu.

Just as I am preparing this reply, I am in receipt of a letter from Dr. Chree, dated December 6, 1910, accompanying a copy of the proof-sheets, which he kindly let me see, of his paper before the Physical Society, November 11; he had also given a paper on the same subject at the British Association meeting. He is not in agreement with my general deductions or with those of Mr. Faris. His criticisms are in part covered by the foregoing reply to Dr. Krogness, and in part by my article in the December (1910) issue (*loc. cit.*). I can only say here that I cannot agree with Dr. Chree in several of his own deductions, and especially with regard to the possible inaccuracy of Mr. Faris's time scalings: I beg to refer him to pp. 213-4 (*loc. cit.*). Nor can I enter here into a discussion with regard to his criticisms of my hypothesis of ionic currents, for it would seem that he has unintentionally put into his discussion ideas which are new to me. I will only remark that nowhere in my papers have I supposed such a simple overhead electric current in the plane of the geographical equator as postulated by him; this is best shown by my mathematical analysis.

In conclusion, I would like to state my position once more, viz. *even our most sudden magnetic storms begin at measurably different times for various stations distributed over the globe. The data thus far available would show that the Greenwich mean times of beginning increase more often in an eastward direction than in a westward one.*

Our explanations as to the cause may differ, but I believe what I have just stated to be an actual fact.

L. A. BAUER.

Washington, D.C.,
December 19, 1910.

Tribo Luminescence of Uranium.

MOISSAN first directed attention to the pyrophoric properties of metallic uranium. The luminosity shown on shaking a bottle containing metallic uranium is due to the oxidation of small particles of the metal. Uranium is a hard but brittle metal; when pieces of it rub together small particles are knocked off, and if these are neither too small nor too large the friction may be sufficient to heat them above 170°C. , at which temperature uranium inflames in air. The presence of smaller particles, which do not inflame visibly in air, is shown by their incandescence in a gas flame lit by the "spark" from the metal. The luminosity obtained by rubbing metallic uranium is not the same class of phenomenon as the luminescence produced by shaking a tube containing uranium nitrate; the latter has been described as tribo luminescence (Wiedemann). If the tube containing metallic uranium is filled with hydrogen no luminosity is obtained, whereas the luminescence of the uranium nitrate is unabated in such an atmosphere. The sparks obtained from uranium are hot enough to kindle a gas flame or explode a mixture of hydrogen and oxygen; in fact, I have been able to work a petrol engine by igniting the gas charger by means of such sparks. The luminescence

of the uranium nitrate crystals, on the other hand, is unaccompanied by any considerable rise in temperature. Pyrophoric properties similar to uranium are shown to a remarkable extent by Welsbach's alloys of rare earth metals and iron.

Tribo luminescence is shown by a large number of organic and inorganic compounds, e.g. arsenic trioxide, uranium nitrate, potassium sulphate, zinc sulphide, quinine valerate, aniline hydrochloride, benzoyl β -naphthylamine. Crystallo luminescence, or the luminosity produced during crystallisation, is practically the same phenomenon, being caused by the fracture of crystals after formation; it is well shown by mixtures of sodium and potassium sulphate. Tschugaeff found a connection between the optical activity and the tribo luminescence of organic substances, but Gernez has disputed the existence of any relation between them. Substances that phosphoresce readily under X-rays generally show tribo luminescence, and the connection between the two phenomena is accentuated by the observations of Karl, which show that quite pure inorganic substances do not show tribo luminescence. It is remarkable in view of the radio-activity of uranium that salts of this metal should show phosphorescence and tribo luminescence to such a degree; Karl has found, though, that quite "pure" uranyl acetate does not show tribo luminescence, while Tschugaeff mentions that the chloride and sulphate also do not exhibit this property, though they are all phosphorescent. The tribo luminescence of crystals may be likened—though analogies are dangerous guides to theories—to the bursting of an elastic band with a snap; when the cohesive forces between the molecules of the crystal are overcome the electrons are disturbed, and light waves result, while substances which easily phosphoresce or are radio-active would the more readily have their electrons disturbed.

Mr. Rudge mentions that the yellow oxide of uranium shows slight tribo luminescence; I could only obtain the effect by fairly vigorous rubbing in a mortar, and as the oxide changes to a dark colour with this treatment, the luminosity may be due to oxidation.

Mr. Rudge's letter directs attention to two interesting but distinct phenomena.

ALFRED C. G. EGERTON.

R.M.A., Woolwich.

The Clarification of Liquids by the Process of Tanking.

I SHALL be glad if any of your readers can give me information upon the following problem. In the clarification of liquids by the process of tanking, the settled clear liquid is drawn off from a tap situated on the side of the tank above the muddy layer. When the tap is turned on, does only the liquid above the tap run out or does some of the liquid below the tap run out also? In the special case of tanking oils, there is very little difference in specific gravity between the upper clear layer and the lower muddy layer. Further, how should the outlet be fitted so that on running out the upper layer the lower should remain least disturbed?

ROWLAND A. EARP.

Preston Brook, near Warrington,
December 22, 1910.

The Conduct and Song of Birds.

THIS morning, Thursday, is clear and frosty, but until now we have had constant rain. In spite of this the birds, for three or four days, have been singing as in early spring. The rooks have been visiting their old nests in the elms, and, our gardener assures me positively, have been carrying sticks and repairing their nests; this he has seen himself, and marked as exceptional. I suspect that this (unusual?) conduct and song herald a period of fine dry weather.

F. C. CONSTABLE.

Wick Court, near Bristol,
December 22, 1910.

P.S.—Fine weather here since December 22 until to-day, January 2!